Decalcified in 1-per-cent. solution of nitric acid in water.

Transferred directly to gum-water, soaked a few hours, and then placed in a paper bag surrounded by spirit.

Imbedded in Stirling's machine and cut.

Gum gradually dissolved away in proof spirit.

Mounted, stained or otherwise, in glycerine or Canada balsam.

I have examined the cochleæ of the following mammals:—man, monkey, sheep, dog, cat, rat, guineapig, rabbit, porpoise, kangaroo. With the exception of the peculiarities in man and monkeys referred to, I have found a striking similarity in the organ of Corti of all these animals.

Unfortunately all my efforts to procure the cochlea of a monotreme have as yet proved unsuccessful, a circumstance much to be regretted, as I fully anticipate that it presents some appearances which link the very dissimilar cochleæ of mammals and birds.

II. "Preliminary Note on the Compound Nature of the Line-Spectra of Elementary Bodies." By J. N. Lockyer, F.R.S. Received January 20, 1876.

In a former communication to the Royal Society (Proc. vol. xxii. p. 380, 1874) I referred briefly to the possibility that the well-known line-spectra of the elementary bodies might not result from the vibration of similar molecules; and I was led to make the remark in consequence of the differences in the spectra of certain elements as observed in the spectrum of the sun and in those obtained with the ordinary instrumental appliances.

I have now clear evidence that the molecular grouping of calcium which, with a small induction-coil and small jar, gives a spectrum with its chief line in the blue, is nearly broken up in the sun, and quite broken up in the discharge from a large coil and jar, into another or others with lines in the violet.

I say "another" or "others," because I have not yet been able to determine whether the last-named lines proceed from the same or different molecules; and it is possible we may have to wait for photographs of the spectrum of the brighter stars before this point can be determined.

This result enables us to fix with very considerable accuracy the electric dissociating conditions which are equivalent to that degree of dissociation at present at work in the sun.

I beg permission to append the following Letter from Prof. Stokes and my reply:—

March 3, 1876.

My dear Lockyer,—You might perhaps like that I should put on paper the substance of the remarks I made last night as to the evidence of the dissociation of calcium.

When a solid body such as a platinum wire, traversed by a voltaic current, is heated to incandescence, we know that as the temperature increases, not only does the radiation of each particular refrangibility absolutely increase, but the proportion of the radiations of the different refrangibilities is changed, the proportion of the higher to the lower increasing with the temperature. It would be in accordance with analogy to suppose that as a rule the same would take place in an incandescent surface, though in this case the spectrum would be discontinuous instead of continuous. Thus if A, B, C, D, E denote conspicuous bright lines, of increasing refrangibility, in the spectrum of the vapour, it might very well be that at a comparatively low temperature A should be the brightest and the most persistent; at a higher temperature, while all were brighter than before, the relative brightness might be changed, and C might be the brightest and the most persistent, and at a still higher temperature E. If, now, the quantity of persistence were in each case reduced till all lines but one disappeared, the outstanding line might be A at the lowest temperature, C at the higher, E at the highest. If so, in case the vapour showed its presence by absorption but not emission, it follows, from the correspondence between absorption and emission, that at one temperature the dark line which would be the most sensitive indication of the presence of the substance would be A, at another C, at a third E. Hence, while I regard the facts you mention as evidence of the high temperature of the sun, I do not regard them as *conclusive* evidence of the dissociation of the molecule of calcium.

Yours sincerely,

G. G. Stokes.

5 Alexandra Road,
Finchley Road, N.W.,
March 5, 1876.

Dear Professor Stokes,—I was not prepared for your suggestion, as it was the abnormal and not the normal behaviour of Ca which led me to investigate it.

D is darker than any other of the Na lines, and H in the chromosphere at the Ca level is red, while in the coronal atmosphere it is green; *i. e.* the *least* refrangible line is developed by increase of temperature, and not the more refrangible one*.

I am not the less grateful to you for your suggestion; and so soon as I can obtain the use of a more powerful coil I will go over the ground as completely as I can.

* [The point, however, is, Which is the *most persistent* line at the respective temperatures, *i. e.* the last to disappear when the quantity of substance present is continually reduced? and Mr. Lockyer himself has shown that the line which is the most conspicuous when there is substance enough present to show several lines is by no means necessarily the most persistent.—G. G. S.]

Are you quite sure that the molecular structure of the platinum wire is constant while it behaves as you say it does?

I beg you will permit your letter and this to appear in the Proceedings. It will make my note more useful if you will.

Believe me

Very faithfully yours,

J. Norman Lockyer.

III. "On the Influence of Coal-dust in Colliery Explosions." By W. Galloway. Communicated by Professor Frankland, F.R.S. Received February 4, 1876.

In coal-mines in which the temperature of the workings is considerably higher than the mean annual temperature at the surface, there is usually a layer of rubbish on the floor of the roadways and air-ways, which consists, in some places, almost entirely of dry coal-dust, and in others of coal-dust mixed with small pieces of coal and stone.

If it could be shown, therefore, that a mixture of air and coal-dust is inflammable at ordinary pressure and temperature, there would be no difficulty in accounting for the extent and violence of many explosions which have occurred in mines in which no large accumulations of firedamp were known to exist; for it is only necessary to suppose that a sudden gust of wind (originated, for example, by the explosion of a small accumulation of firedamp) had swept through the adjoining galleries, raising a cloud of dust into the air, and then all the other phenomena would follow in regular order. The flame of the originally inflammable mixture would pass directly into the newly formed one, expanding its volume; the disturbance would be propagated over an ever-widening area, until that area might possibly become coextensive with the workings themselves; and the consequences would be the same as if the whole space had been filled with an inflammable mixture before the disturbance began.

The accounts of colliery explosions published in this country hardly ever allude to the existence of coal-dust; and when they do so, in one or two cases, it is for the purpose of suggesting that the gases disengaged from it by the heat of the firedamp-flame would no doubt be ignited, and tend to increase the force of the explosion*. This seems all the more

* Since the above was written, my attention has been called to the following observations by Faraday and Lyell (Phil. Mag. 1845):—"In considering the extent of the fire for the moment of explosion, it is not to be supposed that firedamp is its only fuel; the coal-dust swept by the rush of wind and flame from the floor, roof, and walls of the works would instantly take fire and burn, if there were oxygen enough in the air present to support its combustion; and we found the dust adhering to the face of the pillars, props, and walls in the direction of and on the side towards the explosion,